Letters

COMMENT & RESPONSE

Mental Health and the Army

To the Editor The articles from the Army Study to Assess Risk and Resilience in Servicemembers (Army STARRS)[1-4] concluded that soldiers have higher rates of mental disorders than civilians (especially intermittent explosive disorder [IED]), that most conditions preexisted military service, and that Army suicides are a direct result of deployments. As the authors pointed out, these conclusions have important policy implications such as how individuals are selected for service. It is critical to correctly interpret these articles within the context of other large Framingham-like initiatives including the 67-year longitudinal Millennium Cohort study involving more than 200 000 service members and veterans.[5]

The conclusion that the mental health of soldiers is significantly worse than that of civilians is contingent on the civilian comparison group being representative. The National Comorbidity Study Replication subsample the authors[1] chose for comparison is clearly not representative, as indicated by the uncharacteristically low prevalence of several disorders (eg, depression prevalence <1%). National surveys from the Centers for Disease Control and Prevention estimate the prevalence of depression in US men and women ages 18 to 39 years of 5% and 10%, respectively,[6,7] entirely comparable with soldiers (4.8%). The very low National Comorbidity Study Replication posttraumatic stress disorder prevalence (0.6%) is another example of an unreliable estimate (further complicated by the soldier survey including a nonstandard 9-item measure rather than the Posttraumatic Stress Disorder Checklist specified by the authors).[1,8]

The conclusion by Kessler et al[1] that IED is the most common mental disorder in US soldiers (11%) is not supported. The IED diagnosis is very infrequently used in military and civilian settings (eg, <0.1% of mental health diagnoses involving soldiers[9]). Problems with the clinical validity of IED have led to extensive revision in the *DSM-5*. Intermittent explosive disorder–related questions on Army STARRS surveys likely reflect anger and aggression behaviors not specific to IED (ie, "attacks of anger" [without good reason or unable to resist] that involve breaking or smashing something, hitting or trying to hurt someone, or threatening someone).[8] The low number of attacks considered sufficient to meet the definition in Army STARRS fails to approach current *DSM-5* criteria. These behaviors are prevalent in the general population, and it is a mistake to conclude that soldiers have greater propensity toward them. For example, the Centers for Disease Control and Prevention Youth Risk Behavior Survey[10] found that 34% of male and 19% of female 12th graders, the population from which the Army recruits, have been in a physical fight in the past 12 months. In addition to the definitional problems, the authors did not consider the context of certain aggressive behaviors (eg, combat environment or combatives training), the well-known associations with deployment-related mental health problems,[11] or the fact that aggression is a frequent clinical focus during treatment of posttraumatic stress disorder, bipolar disorder, attention-deficit/hyperactivity disorder, and many disorders not assessed in these Army STARRS publications (eg, adjustment disorders, sleep disorders, personality disorders, alcohol or substance withdrawal, and traumatic brain injuries). The finding by Nock et al[2] of the strong association of IED with suicide attempts also supports the conclusion that IED survey items actually measured nonspecific anger-related behaviors inherent to numerous conditions.

The conclusion by Kessler et al that more than 75% of soldiers who met criteria for a mental disorder experienced onset prior to coming to the Army[1] is also unreliable. For example, time at onset for IED was determined using a single question that asked soldiers who screened positive for IED how old they were the first time they had an anger attack[8] (which was compared with service entry date). Similar approaches were used for the other conditions. Certainly this is not sufficient to conclude that a disorder preexisted service or that the level of impairment was sufficient to meet diagnostic criteria or prohibit military service.

Schoenbaum et al[3] suggested a direct association between deployment and suicides not found in the Millennium Cohort study[5] and hypothesized that study differences stem from service differences, response biases, or "people with emotional problems" having lower rates of survey participation. A more likely explanation not mentioned by Schoenbaum et al[3] is that the risk-factor analysis only presented unadjusted bivariate associations, whereas the Millennium Cohort study conducted multivariate analyses (linking Department of Defense records with national death records) that controlled for important demographic differences, combat exposure, and mental disorders.[5,12] Nock et al[2] also failed to control for combat and underlying mental disorders in their analysis of deployment and suicide attempts.

The figures in the article by Schoenbaum et al[3] showed Army suicides increasing just as sharply during the study years in soldiers who never deployed as in currently or previously deployed soldiers. These data clearly show that deployments do not directly explain the sharp increase in suicide incidence. This is not to suggest that the extended conflicts in Iraq and Afghanistan have had no cumulative impact. War-zone deployment, particularly combat intensity, has contributed to an increase in mental disorder prevalence but prevalence has also risen overall in the population; the presence of mental disorders is the strongest predictor of suicide.[5,12] In other words, deploying to a war zone itself does not necessarily confer increased suicide risk but increased risk does occur for individuals who develop mental disorders from deployment or nondeployment stressors.

With the conclusion of operations in Iraq, the drawdown in Afghanistan, and the projected Army downsizing, we ex-

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pect to see gradual declines in the prevalence of mental disorders and suicide incidence. This first set of publications from the $65 million Army STARRS epidemiologic effort presents a misleading perspective of the mental health of US soldiers and has the potential to misinform public policy, health care strategies, and military accession standards. We hope future Army STARRS publications will better integrate findings with existing research and contribute to actionable recommendations.

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Disclaimer: This letter reflects the views of the authors and is not the official position of the US Army or Department of Defense.

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To the Editor The underlying assumption that personal weakness rather than war is responsible for the high rates of mental health disorders among military personnel ignores historical data and raises questions about the study and its conclusions by Kessler et al[1] published in *JAMA Psychiatry*. Retrospective analyses of premilitary, military, and postmilitary risk factors conducted since World War I almost uniformly conclude that the single best predictor for stress casualties is cumulative exposure to war stress followed by perception of low social support.[2]

In preparation for World War II, the US Army screened out 1.6 million predisposed war neurotic individuals, an unprecedented psychiatric screening that far exceeded traditional risk factors (eg, parental mental illness, parental divorce, weak physical appearance, virginity, and grade failure) and the United States entered the war with the most physically and psychologically healthy/resilient cohort in human history.[3] However, in 1943, the screening program was declared a dismal failure by the Army chief of staff as 1.1 million of America's most resilient and nonvulnerable soldiers were neuropsychiatric patients.[3] The Army subsequently enlisted some with known risk factors and found no distinction between these cohorts and their healthy, resilient counterparts. Additionally, the Army's studies of hundreds of stress casualties among battle-tested, award-winning, senior military leaders—or Old Sergeant Syndrome—found that some had preexisting mental health problems, while many did not.[4]

A high incidence of war-stress wounds has historically spurred reflexive military worries over malingering and mass evacuation syndromes that imperil its ability to fight and win wars, coinciding with government concerns over escalating rates of disability pensions threatening to bankrupt the homeland.[5] Consequently, the authenticity of war-stress injuries, as commensurate with physical injuries, is discredited, citing preexisting individual weakness and inadequate screening, training, or leadership. Rather than arguing the credibility of war-stress injuries, reducing the stigma of mental disorders, whether acquired before or during military service, is essential to avoiding future crises.

At the end of World War II, Army leaders rediscovered the critical war trauma lesson: "When finally, psychiatric casualties were regarded as legitimate consequence of battle stress and strain, it became possible to prepare adequately for their prevention and treatment."[6]

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To the Editor We read with interest a series of reports by Schoenbaum et al,[1] Kessler et al,[2] and Nock et al[3] but wondered where gender identity disorder (GID) fits in the picture. The authors described the predictors, prevalence, and correlates of suicidal risk, ideation, and behavior among servicemembers who completed the Army Study to Assess Risk and Resilience in Servicemembers (Army STARRS). These reports concluded that, given the large proportion of post-enlistment suicide attempts, pre-enlistment mental disorders might be targets for screening for suicidality and potential motivations for intervention. According to Kessler et al,[2] there is reason to believe that comorbidity is likely to influence suicide risk and specific subpopulations may have elevated risks.

In our opinion, one such subpopulation within the US military that is particularly vulnerable to suicide risk is the transgender community. Transgender-identified servicemembers and veterans comprise a large proportion of the general transgender population. In fact, some research findings and clinical observations suggest that transgender military service is elevated compared with the general population.[4] Consequently, a proportion of transgender servicemembers and veterans meet criteria for GID.[5]

Among servicemembers and veterans, GID is an increasingly diagnosed condition.[5] A 2011 review of Veterans Health Administration data showed that 22.9 of every 100 000 Veterans Health Administration users had been diagnosed as having GID—a rate 5 times higher than the general population (approximately 4.3 of every 100 000).[5] Gender identity disorder has been significantly associated with suicidal ideation and behavior.[5] Thus, being a member of the US military and identifying as transgender potentially constitutes a double risk for suicide. For example, between 2000 and 2011, rates of suicide-related activity for veterans with GID were 20 times higher than for the general veteran population.[5]

Unlike other psychiatric disorders where the aim of treatment is to diminish the symptoms, GID treatment is the exacerbation of symptoms (eg, living full time in cross-gender role and gender reassignment surgery). Decreasing suicide risk among those with GID in the military warrants more attention regarding etiology, prevention, and intervention. While the Army STARRS studies on suicide conclude that pre-enlistment mental disorders might be better screened, an alternative strategy to reduce the risk for suicide among post-enlistment servicemembers may include changing policies to allow transgender men and women, including those with GID, to openly serve in the US military. Doing so might help reduce suicide risk among a currently hidden population that is likely to contribute to the elevated statistics of suicide risk, ideation, and behavior, yet is unable to seek treatment and openly serve under current military policies.

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In Reply On behalf of the Army Study to Assess Risk and Resilience in Servicemembers (Army STARRS) Collaborators, we would like to address the 3 main criticisms of our articles[1-3] by Hoge et al in their letter. As detailed here, all available data suggest that these criticisms are without scientific merit. Hoge et al also made a number of secondary criticisms that, like the primary criticisms, are without merit. However, we focus here only on the 3 main criticisms: (1) that we were incorrect in asserting that soldiers have higher rates of current mental disorders than comparable civilians; (2) that we were incorrect in asserting that most soldiers with current mental disorders had first onsets prior to enlistment; and (3) that we were incorrect in concluding that Army suicides are a "direct result" of deployment (a criticism of something we did not say).

Regarding the higher rates of mental disorders among soldiers than civilians, we found that, consistent with the claim in our *JAMA Psychiatry* article,[1] soldiers do, in fact, have higher rates of mental disorders than comparable civilians. It is critical to be clear on the term *comparable* though because there are many noncomparable samples. Hoge et al compared the depression prevalence rate in the Army STARRS survey with the rate in a Centers for Disease Control and Prevention survey where roughly one-fourth of respondents had less than a high school equivalent education (making them ineligible for

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Army service) and half were women (compared with roughly 15% in the Army), leading to a substantially inflated rate of depression compared with the rate that would have been obtained if the survey composition had been adjusted to be comparable with that of the Army. By contrast, our comparison sample was carefully constructed from the nationally representative National Comorbidity Survey Replication to be identical to the active-duty Army population on the joint distributions of sociodemographic variables and to exclude people with serious health problems that are exclusions for Army service. Soldiers in the Army STARRS survey had substantially higher rates of current mental disorders than respondents in that representative comparison sample.

We also reported that most active-duty soldiers with current mental disorders had onsets of their first mental disorders before their age at enlistment.[1] Hoge et al based their criticism of this conclusion on our use of retrospective reports to define age at onset. This criticism ignores 2 important points. First, an extensive literature cited in our article, but ignored by Hoge et al, documented that prospective data converge with retrospective data in finding early age at onset distributions of mental disorders consistent with those found in our report.[4] Second, Army STARRS used the same assessment methods in a separate survey of approximately 57 000 Army recruits who had just begun Basic Training.[5] We found high rates of prior lifetime mental disorders in that survey of new recruits of the sort implied by the retrospective results in the articles critiqued by Hoge et al, providing strong support for our claim that most soldiers with current mental disorders had first onsets before enlistment.

Finally, Hoge et al stated incorrectly that we claimed Army suicides are a "direct result" of deployment. We made no such claim.[3] Indeed, we stated clearly that causal interpretations of suicide trends cannot be made from the naturalistic data we reported. In his editorial about our articles, Friedman[6] emphasized that he was clear on this point. We noted that the overall Army suicide rate over the 2004-2009 period we studied was higher among the currently and previously deployed than the never deployed, but we also noted that the increase in the Army suicide rate over that period occurred not only among the currently and previously deployed, but also among the never deployed. And we reported in another Army STARRS article that the association between deployment history and suicide varies with rank and time in service.[7] For example, the suicide rate of officers is actually higher among the never deployed than the currently or previously deployed. The criticism by Hoge et al misrepresents our findings and interpretations.

Suicides and mental disorders among servicemembers are serious issues that require serious scientific investigation. The challenges involved in research aimed at elucidating the causal mechanisms underlying these outcomes and designing interventions to prevent them from happening are great owing to the complexity and rarity of the phenomena and the difficulties in making plausible causal inferences from data with the range of potential selection biases found here (most notably that risk factors for these outcomes might be related to volunteering for Army service, selection out of deployment once in the Army, exposure to a variety of experiences thought to be risk factors for suicide, and early attrition from Army service). Awareness of these complexities underlies the logic of our analyses and interpretations.

We welcome thoughtful commentaries on this work and are eager to learn of genuine problems with our logic or interpretations, as well as to hear suggestions for better ways to produce actionable recommendations for effective interventions.

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Correction: This article was corrected online August 6, 2014, to omit information from the first paragraph.

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Very Small *P* Values

To the Editor We are deeply concerned about the article by Hartz et al[1] published in *JAMA Psychiatry*. Most of our concerns have to do with the extremely small *P* values reported in the text proper and in the tables. Several of them appeared to be beyond the precision capability of the statistical software (SAS) that was used. For example, in their Table 3, an odds ratio of 3.96 (95% CI, 3.61-4.35) is said to have an associated *P* value of 1.2×10^{-188}. The other 4 *P* values in that table were even smaller. Whether or not those *P* values have been correctly calculated, there is no reason for reporting anything other than $P < .0001$, which is the default of SAS for very small *P* values. (The authors also vacillated between *P* being equal or less than a certain value.) Furthermore, there is no need for both confidence intervals and *P* values. If an odds ratio of 1 is not in-

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